PHILOSOPHICAL EMPIRICISM:

CONTAINING

REMARKS on a CHARGE of PLAGIARISM

respecting Dr. H——s,

INTERSPERSED

With various Observations relating to

DIFFERENT KINDS OF AIR.

By JOSEPH PRIESTLEY, LL.D. F.R.S.

Viritur ex tapto. Non hospes ab hospite tutus.

OVID.

LONDON:

Printed for J. JOHNSON, No. 72, *St. Paul's Church-yard*.

1775.

(Price One Shilling and Six Pence.)

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ADVERTISEMENT.

Though this piece was originally intended to anſwer a temporary purpoſe, it is likewiſe calculated to refute ſome prevailing miſtakes concerning the doctrine of *air*, and therefore will, I hope, be of uſe in eſtabliſhing fundamental and juſt principles in this branch of Natural Philoſophy, which is now become an object of very general attention.

I have not publiſhed the name of my antagoniſt at length, partly becauſe I am really aſhamed of ſuch a conteſt; and alſo becauſe I would not do him any more injury than I was obliged to do in my own juſtification. It will not be expected, I hope, that I ſhould be quite grave and ſerious through the whole of this affair. I have been, I think, ſufficiently ſo at the beginning; but the occaſion did not require it throughout: and, indeed, it was not in my power to treat this very abſurd and ridiculous accuſation, but with a great mixture of ridicule and contempt.

Since the writing of this pamphlet, Mr. Godfrey (of whom I made ſome inquiry concerning the converſion of earth into water, mentioned p. 57) has been ſo obliging as to favour

me with part of a quantity of earth that had been produced from diſtilled water, weight for weight, by that celebrated chemiſt his grandfather, the cotemporary of Mr. Boyle, and his fellow-labourer.

This earth, I find, yields *fixed air* in great plenty, by the heat of a burning lens in quickſilver, as well as by means of the acids. And when it is made into a paſte with ſpirit of nitre, it yields more air, the greateſt part of which is alſo fixed air. This experiment I barely announce at preſent, as exhibiting a new fact reſpecting *the generation of fixed air*, that cannot be publiſhed too ſoon.

THE

THE
INTRODUCTION,

CONTAINING

The LETTERS that paſſed between the AUTHOR, Dr. H——s, and Dr. BROCKLESBY.

IT is with much reluctance, as ſeveral of my friends can witneſs, that, after withſtanding, as long as I could, their earneſt remonſtrances on the ſubject, I have been induced to make this appeal to the public; I having been willing to think it unneceſſary, and they inſiſting upon it that it was abſolutely neceſſary. At length I yielded to their reaſons. The caſe is as follows.

Before I left London, in the ſpring of the preſent year, in which my acquaintance with Dr. H——s commenced and terminated, I was told it was reported, that ſome of my new experiments, of which I had ſent an account to

the Royal Society, ſubſequent to my acquaintance with him, were only the reſult of his *general principles* concerning air; and Dr. Brockleſby, when he ſaw ſome of my new experiments, in the company mentioned in my letter to him, ſaid of them *all*, without diſtinction, that they were thoſe that Dr. H——s had ſhewn. But as I knew that Dr. H——s and myſelf held no common principles concerning air, as Dr. Brockleſby had not the character of being the moſt accurate man in the world, and I thought that my character for *veracity*, at leaſt, was ſufficiently eſtabliſhed, I intirely neglected the inſinuation, and really thought no more about the matter, till I was informed, by a letter from London, while I was in the country, that the report of my having taken ſeveral things from Dr. H——s gained ground.

Knowing, however, that there could be no foundation for this charge, I continued to pay no attention to it; and though, upon coming to town, I found it was in every body's mouth, and my friends urged me to make ſome inquiry concerning it, I neglected to do it for a conſiderable time; thinking that the publication of my *ſecond volume of Obſervations concerning Air*, which was then nearly printed off, would ſpeak for itſelf, and ſatisfy every body who

who ſhould peruſe it, that the narrative carried its own evidence along with it.

But I was told that the charge of plagiariſm, abſurd as it was, had been ſo long, and ſo induſtriouſly circulated, without having been contradicted by any proper authority, that it had really gained much credit; that many perſons, without diſtinguiſhing times or dates, had publicly, and with great confidence advanced, that even *all* my diſcoveries had been taken from the ſame Dr. H——s. On this account, not only my friends, but perſons with whom I had no ſtrict connection, aſſured me that, in their opinion, it really behoved me to make ſome regular inquiry into the buſineſs. Accordingly I did, at length, though with great reluctance (ſtill hoping that there could be no neceſſity for any appeal to the public upon the ſubject) ſet myſelf about it; when I preſently found what the following letters will ſpecify.

To Dr. Brocklesby.

Dear Sir,

The buſineſs I write to you about is ſo irkſome to me, that I have deferred it as long as poſſible, hoping there might be no occaſion to

give you any trouble on the ſubject. At length, however, I have been perſuaded by my friends to do it.

It is reported, I find, that ſome experiments, which I have lately exhibited as my own, I took from Dr. H——s, and wherever I inquire about it, I am told that you charged me with it when you ſaw my experiments at Shelburne-houſe, in company with Dr. Fothergill, the two Dr. Watſons and Doctor and Mr. John Hunter*.

Now as you did not at that time charge me with any plagiariſm, but only ſuppoſed that both Dr. H——s and myſelf had made the ſame diſcovery, and did not even ſay that you had yourſelf ſeen thoſe experiments of Dr. H——s's, I muſt beg the favour of you to tell me what thoſe *common experiments* were, and by what authority you took upon yourſelf to ſay, that the experiments you then ſaw were the ſame with thoſe of Dr. H——s's, which you had not ſeen; for, if I remember right, I ſhewed you *ſeveral* at that time, which were not mentioned in my firſt volume.

I have not heard that Dr. H——s himſelf charges me with having taken any thing from him;

* This was on the 23d of May, 1775.

him; and with reſpect to the principal thing which I then ſhewed you as new, it is impoſſible that he ſhould have claimed it; when, as I immediately told you, in the preſence of the gentlemen abovementioned, it was but a little time before, that he had heſitated to admit the facts when I mentioned them to him; as, indeed, I ſhould have done myſelf a little before that, had any other perſon mentioned them to me; the diſcovery of them having been perfectly accidental, and affording no foundation for merit whatever.

What he advances in his printed *Syllabus* is the very reverſe of my ideas on that ſubject, and, in my opinion, is contradicted by the experiments I then ſhewed you. Indeed, it is now abundantly evident, that Dr. H——s and myſelf have hardly one common idea concerning air; ſo that if he be right, moſt of my diſcoveries are, what he has thought proper to call them, mere *conceits*; and if I be right, his general doctrine is entirely chimerical and falſe. On this account, it is hardly poſſible that we ſhould have taken any thing from each other; except that he has adopted ſome things contained in my *firſt volume*, the ſecond edition of which had been publiſhed ſome time before I had ſo much as heard the name of Dr. H——s.

In this buſineſs, therefore, there muſt have been ſome miſtake (I hope not yours) which I am told it behoves me to inquire into. I am, with real regard,

Dear Sir,

Your very humble Servant,

Shelburne-houſe,
Nov. 30, 1775. J. PRIESTLEY.

This letter I delivered to the Doctor at the Royal Society, on the day in which it is dated; and the ſame day, having received farther information concerning the buſineſs, I wrote the following letter to Dr. H——s.

Sir,

I have this day been informed, from undoubted authority, that you have charged me with having publiſhed, as my own, experiments, what I learned of you; but though I have inquired of ſeveral perſons, who all agree in the fact, of the charge in general, none can tell me what the particulars of it are. I muſt, therefore, beg that you would yourſelf inform me concerning them. A man of honour would have given me an opportunity

tunity of vindicating myſelf, before he had publiſhed my accuſation to others.

I am, Sir,

Your very humble ſervant

Shelburne-houſe,
Nov. 30, 1775. J. PRIESTLEY.

The next day I received the following anſwer from Dr. Brockleſby, and on Dec. the 3d, that which follows from Dr. H——s.

Dear Sir,

The experiments which I ſaw you inſtitute at Shelburne-houſe appeared ſo nearly the ſame with a greater variety of ſuch as I had ſeen in three courſes of chemiſtry given by Dr. H——s, that, in juſtice to my abſent friend, I was urged, poſſibly, to violate the laws of hoſpitality, by declaring in the inſtant, that none of the divers experiments you was then pleaſed to exhibit were novel to me, except one concerning the Swediſh fluor.

Whether your diſcoveries were prior to thoſe of Dr. H——s I muſt leave to the determination of others, it being, at this diſtance of time, not eaſy for me to aſcertain to whom the priority of theſe claims belongs.

Whenever this ſubject has occurred in converſation, I have repeated what I had, with the moſt pure intentions, declared in your preſence; never apprehending you had cauſe of offence, on ſubjects wherein, by your own declaration, you and Dr. H——s entertained notions totally repugnant.

I ſincerely wiſh your philoſophical improvements may obtain every merited honour: at the ſame time I ſhould feel myſelf unjuſt to ſuppreſs candid applauſe to another gentleman, of whoſe unwearied labours I have been a conſtant witneſs more than a year and half paſt. I am, with great reſpect,

Dear Sir,

Your moſt obedient humble ſervant

Norfolk-Street,
30 Nov. 1775. RICHARD BROCKLESBY.

Sir,

Nine months are elapſed ſince I informed you, in plain, but the leaſt offenſive terms, that I wiſhed to decline your viſits and correſpondence. You know the motives of a conduct ſo candid, and with all ſo repugnant

to my own intereſt as a teacher. You now cannot ſeriouſly expect that I ſhould repeat what you well know — that I ſhould enumerate the things which you aſſume as your own, and which I had previouſly ſhewn and taught.

If any other gentleman had propoſed the queſtion contained in your letter, an anſwer would be neceſſary; and I ſhould commence it with compariſons of the dates of Dr. Prieſtley's rapid publications, with the dates of my courſes of chemiſtry.

For the future I will add to the charge againſt you, that you have treated others as you have treated me; and that your originality in experiments conſiſts chiefly in the knack of rendering the phenomena, which all practical chemiſts have obſerved and underſtood, perfectly myſterious and ſurprizing to others.

The only part of your letter, then, which requires an anſwer, is that wherein you hint that a man of honour would remonſtrate to you, inſtead of uttering the truth to others. Herein your notions of honour and mine differ widely. I ſpeak freely ſuch truths as can be well vouched, but I never remonſtrate, except

cept when a gentleman has inadvertently offended.

I am, Sir,

Your humble Seruant

Greek-ſtreet, Soho,
Dec. 3, 1775. BRY. H——s.

This letter (the groſs rudeneſs, manifeſt ſhuffling, and abſurdity of which, will hardly impreſs my reader in his favour, and may, perhaps, make ſome of his friends bluſh for him) giving me no ſort of ſatisfaction with reſpect to the particulars of the charge of which I was in queſt, I thought it neceſſary to interrogate Dr. Brockleſby more diſtinctly; eſpecially as he owned that he had of *himſelf only*, and not, as I had imagined, through the medium of ſome third perſon, aſſerted the identity of my experiments with thoſe of Dr. H——s. I therefore ſent him the following letter, which brought an anſwer not more ſatisfactory than the former, except that I was convinced by it, that nothing more ſatisfactory could be procured on the ſubject.

Dear Sir,

I am glad to find by your letter, that I am to look no farther than to *yourſelf* for the evidence

dence of the experiments I ſhewed you at Shelburne-houſe having been the ſame with thoſe Dr. H——s had exhibited before that time. And as you have not yet anſwered the queſtion which I took the liberty to propoſe to you (ſince experiments that *appear nearly the ſame* with others, may, in reality, be eſſentially different from them) and as Dr. H——s himſelf has refuſed to give me any ſatisfaction on the ſubject, I am obliged to repeat my requeſt. But to make the trouble of ſatisfying me more eaſy to you, I ſhall be a little more particular in this letter than I was in my laſt.

The firſt experiments that I had the pleaſure of ſhewing you were thoſe by which I ſhew in what manner to apply the teſt of nitrous air, to aſcertain the purity of atmoſpherical air, which is deſcribed in my firſt volume, and the manner of firing inflammable air with, or without common air, which I learned of Mr. Cavendiſh. None of thoſe, therefore, are to the preſent purpoſe. After this, the only thing I exhibited, which I declared to have diſcovered poſterior to the publication of my firſt volume (excluſive of the experiments on the *fluor acid*, which you acknowledge you had not ſeen with Dr. H——s) were

were experiments relating to quite *another kind of air.*

Now as, by your own account, you were able to pronounce immediately upon the identity of thoſe experiments with thoſe you had ſeen of Dr. H——s's, and have repeated the ſame thing whenever the ſubject has occurred in converſation ſince, you muſt be able to tell me *now* what thoſe experiments were. Pleaſe, therefore, to anſwer the following queſtions.

1. From what materials did I tell you that I procured that air?

2. What name did I give to it?

3. What were the peculiar properties of it?

4. In what manner did I demonſtrate thoſe properties?

I propoſe theſe queſtions ſo diſtinctly, becauſe unleſs you can anſwer them with preciſion now, it cannot be thought that you were able to pronounce on the ſubject with ſufficient preciſion before. Pleaſe alſo to tell me, as nearly as you can recollect, how long it was before you ſaw the experiments above-mentioned

mentioned with me, that you had ſeen the ſame with Dr. H——s. Was it in his firſt, in his ſecond, or in his third courſe? for you mention three of them.

I will add, that the experiments which I exhibited to you were ſo very remarkable, and ſo exceedingly different from any others, of which any printed account was then publiſhed, that they muſt have ſtruck you in a particular manner; and therefore you cannot but remember pretty nearly when it was that you firſt ſaw them. I do not even think it poſſible that they could have been exhibited at any public lecture in London, without occaſioning ſo much converſation among philoſophers upon the ſubject, that I muſt myſelf have heard of them.

You need not make any apology for what you call *violating the laws of hoſpitality*, provided you have ſcrupulouſly obſerved (as I am fully ſatisfied you have done *intentionally*) the much more important laws of *truth*.

I am,

Dear Sir,

Your obedient humble Servant,

Shelburne-houſe, J. PRIESTLEY.
Dec. 4, 1775.

THE ANSWER.

Sir,

Your letter found me yeſterday ſent for to attend a lady ill of a fever at Guildford, and I did not return till this day noon, and in the firſt moment of leiſure, at 10 o'clock at night, I now ſit down to anſwer your letter, and I hope it may cloſe our correſpondence on this ſubject of controverſy, from which my temper is truly averſe.

I muſt, in the firſt place, ſubmit to your reconſideration the following paragraph in your letter. "I propoſe theſe queſtions ſo diſtinctly, becauſe, unleſs you can anſwer them with preciſion now, it cannot be thought, that you were able to pronounce on the ſubject with ſufficient preciſion before."

Hereupon, I take leave to obſerve, that this concluſion is not admiſſible, and that an opinion given in the inſtant, and in your preſence, whilſt the facts were before me, may have been altogether juſt, although, at the diſtance of many months (having ſeen, both before and afterwards, frequent and various combinations of ſimilar experiments) I do not perfectly recollect every experiment then made, nor even all the new names you might have

given to appearances, which were familiar to me. I will not, therefore, attempt to anſwer the following queſtions, which might involve me in ſubtilties, or at leaſt lead you to further perplexities, rather than clear up the ſubject.

1. From what materials did I tell you, that I procured that air?

2. What name did I give it?

3. What were the peculiar properties of it?

4. In what manner did I demonſtrate thoſe properties?

But your 5th and laſt queſtion I will moſt readily anſwer. Dr. H——s, in the firſt courſe of Chemiſtry, June 1774, read his firſt lectures on the ſubjects of air, fixed air, inflammable air, the elaſtic matter of acids, of alkalies, of phoſphorus, ethers, and on phlogiſton, light, and fire; on all which ſubjects he entertained (to the beſt of my recollection) the opinions he now advances, and he ſupported theſe opinions by various ſtriking experiments, and by ſome of thoſe you ſhewed, among others. And having ſeen the manuſcripts from which he read in his firſt courſe, and

and which were voluminous, I am perſuaded that very many of his concluſive experiments were made at a period anterior to his firſt courſe. Thus far I ſpeak what is known to others of his pupils; but of my own knowledge I can affirm that, in private converſation, he has repeatedly diſcuſſed, and debated theſe ſubjects with me, ſo as to have converted me from my formerly-imbibed opinions of fixed air, inflammable air, and phlogiſton, for ſeveral years previouſly; and I remember particularly his converſations concerning Mr. Woulfe's method of ſaving the acid, ethereal, and alkaline elaſtic fluids, publiſhed years ago; in divers of which converſations he attempted to convince me of the nature of theſe fluids, always expreſſing the higheſt veneration of his favorite philoſopher Mr. Cavendiſh, whoſe genuine taſte and preciſion in conducting experiments, and his philoſophical inductions, he was often wont to ſay were truly worthy of a diſciple of Bacon, or the immortal Newton; and that modern Philoſophy, in his opinion, owed more to Mr. Cavendiſh, than to any other man now living, except Dr. Franklin. In conſequence of a variety of thoughts, ſuggeſted to him, by a careful peruſal of Mr. Cavendiſh's works, Dr. H——s, in his firſt, as well as in his ſucceeding courſes, brought experiments concluſive with me, ſo that I feel

feel myſelf as much convinced, as the nature of theſe recondite matters admits of, that his notions of elaſtic fluids, diſtinct from air, are founded in nature, and that acid, whether vitriolic, muriatic, or vegetable, is an elaſtic fluid when detached, and that, even however combined with phlogiſton, all theſe, together with the microcoſmic acid, may form a combuſtible vapor, incoercible in the ordinary proceſſes of chemiſtry, but which may be detained in proper veſſels to ſerve for experiments.

And I apprehended that ſuch combuſtible vapour (whether in making ether, or metallic ſolutions, or by decompoſing ſulphur with iron filings moiſtened with water, or if even Knuckel's phoſphorus, formed or detached by various other artifices, deviſed by Dr. H——s, to confine phlogiſticated vapour) will burn in open veſſels, in that part immediately in contact with the atmoſphere; and I learned that theſe elaſtic vapours when mixed in various proportions with common air, and approached by flame, ſhall diſplode, and cauſe a loud noiſe in going off, and leave the air newly combined with ſome principle that was in the veſſel, ſo that it ſoon ſhall become fixable air; in almoſt all which he candidly repeated his obligations to Mr. Cavendiſh.

The acid of nitre he all along conſidered nearly in the ſame manner as the other acids, with this difference only, that with the elementary acid of nitre he ever impreſſed the notion, that ſomewhat which operates like air in all combuſtions, and on phlogiſtic bodies, or poſſibly that air itſelf is combined. This too he has ſhewn by experiments with nitrous acid and ſpirits, oils, phoſphorus, metals, &c. with all which viſible fire is produced by his curious proceſſes. And with a number of other bodies only heat, not fire, was produced.

He alſo frequently remarked the phenomena of mixing air with the nitrous vapor, which he did in a very ſimple manner, by only unſtopping the bottle of his ſtrongeſt nitrous acid in a quieſcent air, or remarking the like appearances in a proceſs for pirmieſon, and ſeveral others.

He demonſtrated that ſal-ammoniac is made by combining volatile alkali with muriatic acid, and that this combination takes place in the great elaboratory of nature, in the volcanos of Etna, and wherever elſe that ſalt is found in nature, as well as the proceſſes of art for making this great article of commerce. And that in every poſſible combination of acid va-

por

por with volatile alkali in vapor ſome neutral ſalt is produced. This he alſo explained by forming at his lectures the elaſtic vapor of ſtrong acid and alkali.

Now having fairly given you this minute detail of ſuch experiments made in Dr. H——s's courſe of lectures, aſk yourſelf if thoſe you was pleaſed to exhibit at Shelburne-houſe could appear altogether novel to me: for I apprehend your giving other names to ſuch experiments, or uſing a ſmaller or neater apparatus, did not conſtitute any important new diſcovery.

I will now end this very irkſome buſineſs, with one remark, that the moſt ſublime philoſopher, who weighed diſtant worlds as in a balance, and taught wondering mortals many of the moſt ſecret laws of nature, as they operate on all matter, had ſo great an averſion to dealing in controverſy, that I know, on good authority, the world had like to have been deprived of the Principia, when he apprehended the publication of that book might involve him in any altercation with his cotemporaries; whilſt, in our days, on the contrary, I am, againſt my will, drawn into this long and tedious letter, to ſettle whether a philoſopher, high in modern rank, has the

excluſive privilege in this or that phlogiſticated vapor of the mineral, vegetable, or animal kingdoms. I know this is my firſt literary diſpute, and that it ſhall alſo be my laſt, for I will ſay no more, but that I am,

Dr. PRIESTLEY'S

Humble Servant,

Norfolk-ſtreet, 5th Dec. or rather 6th, at 2 o'clock morn.

RICH. BROCKLESBY.

From this letter it is but too apparent, that Dr. Brockleſby had not been able to diſtinguiſh what he ſaw with me from what he had ſeen with Dr. H——s, and therefore that no ſort of ſtreſs can be laid on his teſtimony. Had I urged him any farther, and (like Daniel with reſpect to Nebuchadnezzar) told him what he himſelf had quite forgotten, or rather had never rightly apprehended, viz. that he had ſeen with me a ſpecies of air which I had procured from earth and ſpirit of nitre, and which I had called *dephlogiſticated air*; being about five times as pure as common air; that a mouſe had lived in a quantity of this kind of air five times as long as it could have done

done in an equal quantity of common air; that a candle burned in it with five times as great ſplendor as in common air; and that when a quantity of inflammable air was fired in it, the report was even fifty times louder than it was in common air; ſtill, ſituated as he was, and ſo little able (as his letter demonſtrates) to diſtinguiſh what he ſaw, he might have perſiſted in what he had incautiouſly once aſſerted, and therefore, without the leaſt violation of integrity; might have affirmed that he had ſeen all thoſe things with Dr. H——s; though according to his own Syllabus, there could not, in nature, be any ſuch thing. But I was far from wiſhing to puſh the Doctor upon this precipice. All I had occaſion for was barely to ſet aſide his teſtimony againſt me, for which his preſent utter ignorance of what he ſaw with me (though things of ſo very remarkable and ſtriking a nature) is abundantly ſufficient.

I muſt not cloſe this article without congratulating Mr. Cavendiſh on his acquiſition of the profound admiration of ſo competent a judge of philoſophical merit as Dr. H——s. But though he knows that I believe him to be very deſerving of the encomiums that Dr. H——s, and Dr. Brockleſby have paid him, I rather think that his feelings upon the occa-

ſion will not be very different from thoſe of Dr. Franklin, in a ſituation that will be mentioned hereafter, and that it would have given him more pleaſure *laudari a laudato viro.*

Finding myſelf, after all the pains I had taken, to lie under an accuſation of ſo very vague and undiſcribed a nature; having endeavoured in vain to procure a *copy of my indictment*, either from my accuſer, or the witneſs; and not knowing how far this unknown charge may extend, I muſt endeavour to make it out myſelf, in the beſt manner that I can, from ſuch materials as the recollection of the whole of my intercourſe with Dr. H——s can ſupply me with; for which purpoſe I muſt go over it all, and eſpecially our converſations on philoſophical ſubjects. This plan will oblige me to mention ſeveral things which muſt appear to his diſadvantage, and which I ſhould not otherwiſe have mentioned. But my ſituation is ſuch, as does not allow me to have recourſe to any other method, more favourable to him. Had his accuſation been *diſtinct*, and confined to any certain number of articles, I ſhould have anſwered to thoſe articles only.

SECTION

SECTION II.

A general account of my intercourſe with Dr. H——s.

It was ſome time in January, of the preſent year 1775, that, being at the Royal Society, I firſt heard the name of Dr. H——s from Dr. Brockleſby, who told me that he was a perſon highly deſerving my notice, as an excellent chemiſt, and eſpecially as one who had made ſeveral diſcoveries concerning *air*. I aſked him what particular diſcoveries, of value, he had made. He replied that he had diſcovered fixed air to conſiſt of common air and phlogiſton. I anſwered, that that was very far from being my idea of the matter, and freely intimated to him, that a perſon who maintained an opinion ſo contrary to all probability could not be much of a philoſopher, or have given much attention to the ſubject. Still, however, the Doctor preſſed me to be introduced to him, and, with much reluctance, as he can witneſs, I did, at length, conſent to dine with him on the day that Dr. H——s was to open his next courſe of lectures, which was the 6th of February following, that we might go together.

In the mean time having inquired of a very reſpectable friend whether he knew any thing of this Dr. H——s, whom Dr. Brockleſby had recommended to me, he adviſed me to have nothing to do with him. Upon this I gave over all thoughts of attending his lecture, as ſome of my friends well know. However, my evil deſtiny, aided a little by *curioſity*, and ſuch a deſire of knowledge, as miſled our firſt parents, helped me, at length, to get rid of my ſcruples; concluding that, though Dr. H——s certainly knew very little about *air*, he might be what is called a good *chemiſt*; and with ſuch a perſon I had long wiſhed to form ſome acquaintance, being conſcious of my own deficiency in that kind of knowledge.

Accordingly, after dining with Dr. Brockleſby, on the day mentioned above, I was introduced by him to this extraordinary man, who received me with marks of the greateſt deference and reſpect, and put me not a little to the bluſh by introducing his compliments to me in the courſe of his lecture, as well as into his converſation,

Upon telling Dr. Franklin, the next morning, where I had been the evening before, he told me that he had once attended one of thoſe introductory lectures of Dr. H——s (four of which

which he gave *gratis* in this courſe) and ſaid, "Pray, did he not pay you ſome compliments "in the courſe of his lecture?" I ſaid yes. He replied, "I thought he would, for he paid me "ſo many, that I was quite aſhamed, and "really had a more unpleaſant feeling, than I "had during all the time of Wedderburne's "lying abuſe of me, before the Privy-Coun-"cil. I believe, however," added he, "that the "man may be a good chemiſt, and his ac-"quaintance may perhaps anſwer your pur-"poſe." Seriouſly, as this great man is now engaged, he will ſmile when he ſees an account of this incident in print, as well as at the ridiculous conteſt into which I have been drawn.

In the manner in which Dr. H——s delivered this lecture there was an appearance of *modeſty* and *diffidence*, with which I was much pleaſed; and, looking upon him as an induſtrious and ingenious man, wholly devoted to his profeſſion, who had expended vaſt ſums of money on his apparatus and experiments; and ſeeming, by his looks, to have waſted his conſtitution, as well as his fortune in theſe purſuits, I really had a ſtrong feeling of compaſſion for him, and made a point of recommending him to my acquaintance, as *a modeſt and ſenſible lecturer*; and this I did pretty warmly

warmly (as I am known to be apt to do, whenever I conceive a liking for any perſon) and this ſeveral of the nobility, other perſons of large fortune and diſtinction, and my philoſophical and literary acquaintance in general, can witneſs. Nor have I, to this day, taken the leaſt pains to unſay any thing that I then ſaid in his favour, or have ſaid any thing elſe to his diſparagement; except when I have been particularly urged to it, by ſomething occuring in converſation, that made it neceſſary for me to do it, in order to my own vindication. Indeed, I was aſhamed to retract what I had, in my incautious zeal, ſo warmly advanced.

In our converſation after the lecture, Dr. H——s, in the preſence of Dr. Brockleſby, expreſſed, in the ſtrongeſt manner, the ſenſe he had of the honour that I did him by my attendance on his lecture, and in a very handſome manner made me a tender of his beſt ſervices, in caſe he could be of any uſe to me. I told him that, not being a practical chemiſt, having never had a proper laboratory, or ſeen much of the uſual proceſſes, I wiſhed to have an opportunity of obſerving ſome of them: but that I more eſpecially was in want of *chemical articles*, ſuch as I could not eaſily procure at the ſhops, or on the preparation of which

which I could not abſolutely depend; and therefore ſhould think myſelf very much obliged to him, if he would ſupply me with ſuch things as I might occaſionally want in the proſecution of my experiments, and that I ſhould very thankfully give him whatever price he choſe to aſk.

This he readily promiſed to do, and added, that if I would do him the pleaſure to call upon him, I ſhould be ſure always to find him at home before dinner, and that there would never fail to be ſome proceſs or other in his laboratory, which I might examine at my leiſure. In return for this obliging offer, I deſired that he would give me the pleaſure of his company at Shelburne-houſe, where I would endeavour, in return, to entertain him with ſuch experiments as I made. But this, alledging he had no time to ſpare, he civilly declined.

From this time I called upon him occaſionally, took of him ſuch articles as I wanted, always gave him his price (concerning which I was intirely ignorant) and always expreſſed myſelf much obliged to him. I ſeldom ſtayed with him more than a quarter of an hour at a time, ſometimes not more than a few minutes; being in haſte to make my experiment with

with the ſubſtance that I procured of him: and I do not think that all the time I ever ſpent with him exceeded four or five hours. Indeed I very ſeldom ſtaid any longer than while he was either finiſhing what I found him about, or while he was employed in weighing, making up, and labelling the ſeveral articles I took of him. Excluſive of this, I do not think that I ſpent more than a ſingle hour with him in all; my own time being as fully employed as his. And the time I ſpent with him in this manner was chiefly out of regard to civility and propriety; thinking it would not be decent to make the ſame uſe of his laboratory, as of a common ſhop; always running away the moment that I had got what I wanted.

The ſecond, which was the laſt time of my attendance on his lecture, I put myſelf to ſome inconvenience to do it, and really did it from no other motive, but that I thought I ſhould oblige him by my countenance; and though I had not the vanity to think that I was doing him all the *honour*, and all the *pleaſure*, that he told me my viſits would do him, I was willing to give him the gratification that he ſeemed to promiſe himſelf from them.

At one time I was induced to make a longer ſtay with him than uſual, by the coming in of Mr.

Mr. Delaval, whom I had not had the pleaſure of ſeeing before, and whom I was much pleaſed with having this opportunity of ſeeing. And I mention it to give my readers ſome idea of the manner in which he, at that time, uſually treated me, that they may compare it with the ſtile of his letter to me.

Upon mentioning my name to Mr. Delaval, which he did in a manner that ſeemed to ſhew he had ſome kind of ſatisfaction in doing it, he ſaid, "You ſee, ſir, all men of note find "me out at laſt," or words to that effect. Alſo, when, in the courſe of one of our converſations, I had occaſion to aſk him whether he happened to have a copy of my book at hand, he replied, with that formality, of which all who are acquainted with him know that he is capable, "Do you think I could poſſibly be "without ſo very capital a performance upon "the ſubject"?

This compliment was, to be ſure, awkward enough; but I did not take it to be meant *ironically*, as there was nothing elſe in the converſation that could bear ſuch a conſtruction. How he can now reconcile theſe encomiums with his calling the principal diſcoveries contained in the ſame book mere *conceits*, and with his ſaying that what I have done conſiſts chiefly

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in *the knack of rendering the phenomena which all practical chemiſts* (and himſelf, no doubt, who is at the head of them all) *have obſerved and underſtood, perfectly myſterious and ſurprizing to others* (that is, thoſe who are not practical chemiſts) I leave to him, as a practical chemiſt, to make out. After theſe compliments, was it poſſible for me to imagine that my company could be ſo very tireſome to him, as he has ſince affirmed?

I had not called upon Dr. H——s more than two or three times, before I began to perceive that his appearance of *modeſty*, and his extreme *deference* and *complaiſance*, began to wear off; ſo that, like the fox with reſpect to the lion, in the fable (if he will like the compariſon) he began to be much more at his eaſe, and his natural character and turn of mind became ſufficiently conſpicuous. For, from an extreme of deference and reſpect, he advanced, by degrees, to ſuch a pitch of *aſſurance*, and ſuch airs of *conceit*, and *ſelf-importance*, as I have ſeldom obſerved in any man; perpetually boaſting of the diſcoveries he had made (but without mentioning any of them) complaining loudly of the great expence he had been at for the ſake of promoting ſcience, and of the low illiberal taſte of the age, diſcovered by his not receiving

ceiving proper encouragement; ſpeaking contemptuouſly of other perſons of his profeſſion, and with particular indignation of many perſons (whoſe names, however, he never mentioned) who had ſtolen their diſcoveries from him, without having made any acknowledgement of it in their publications *.

Such topicks, and ſuch a turn of converſation, into which he was perpetually falling, gave me, I own, no very favourable idea of my new acquaintance. But ſtill I made allowance for this conceit, and bore with it, as being, in ſome meaſure, incident to perſons who give their whole attention to a ſingle thing, in which they are allowed to excel, who have not ſeen much of the world, and who have, therefore, had no opportunity of acquiring that *liberal turn of mind*, which is the greateſt ornament of true ſcience.

* I always joined with Dr. H——s in condemning this kind of conduct, and aſſured him that whatever obligagation I ſhould be under to him, I ſhould certainly acknowledge it; and my book will prove that I have done ſo in the ampleſt manner. My ſecond volume, which is now printed off, was written at a time when I was very far from being ſatisfied with his conduct, though I had not heard of his claims upon me. But though he has uſed me very unhandſomely, I have ſome obligation to him for the materials he allowed me to purchaſe of him, and therefore I do not wiſh to retract what I have ſaid.

I now

I now come to the cataſtrophe of our acquaintance, of which he has given very different accounts, and concerning which I have formed different conjectures, in conſequence of viewing it in different lights, as I ſhould do any remarkable appearance in philoſophy. As I always told him, when I applied to him for any ſubſtance, or preparation, what I wanted it for; I ſometimes aſked him whether he could not recommend to me ſomething elſe that was likely to anſwer my purpoſe better; and ſometimes he would tell me, and ſometimes he declined it; almoſt always concluding the converſations we had upon theſe ſubjects with telling me that *I muſt attend a complete courſe of chemiſtry*. I always replied, that I had not *time* for it; never ſuſpecting what he was aiming at all the while; till, at length, upon his urging me on this head more ſtrongly than before, and my telling him more peremptorily than before, that I really could not ſpare time for any ſuch thing, he ſaid very abruptly, that "his time was ſo much taken "up with neceſſary buſineſs, that, without "meaning any perſon in particular, he was "obliged to come to a general reſolution, "*to anſwer no queſtions but ſuch as he was paid* "*for*." This, in a moment, diſcloſed to me (as I then concluded) what I was aſtoniſhed I ſhould not have diſcovered before, viz. that

his little object had been to get my ſubſcription for attending his courſe. Diſconcerted as I was, I had the preſence of mind to commend his reſolution, as very neceſſary for a perſon of his many engagements; and after this I called upon him no more.

Till this laſt converſation, which was in his own houſe, while he was ſhaving himſelf, and conſequently did not engroſs any of his valuable time, I had ſeen nothing in his behaviour (making the reaſonable allowances above-mentioned) that could give me offence, nor did I perceive any mark of his having conceived the leaſt diſlike to my viſits. Even this very laſt time that I was with him, part of his converſation was, to all appearance, very friendly. He then mentioned to me, particularly, *Mr. Wilſon's book on Phoſphori*, and expreſſed the ſtrongeſt diſapprobation of his treatment of me in it; ſaying he *hated ſuch things* among philoſophers; and added, that he had freely told a friend of Mr. Wilſon, who would be ſure to tell him again, that, beſides the malice of the thing, he was quite wrong with reſpect to the fact.

Could I imagine that a man who talked to me in this manner was, at the ſame time, wiſhing to get rid of me? I therefore conclude, that

his determination was occaſioned by the converſation that immediately followed this, and by which he found that I abſolutely refuſed to attend his lecture; whether his view was merely to get my *ſubſcription money*, which I then imagined to be his object, as thoſe of my friends to whom I told the ſtory can witneſs; or whether he meant to engage my attendance upon his lecture with a view to ſomething farther, as I now conjecture, viz. that he might have the honour of being my inſtructor, and thereby have a pretence for laying claim to all my experiments.

That I *took up too much of his time*, I am ſatisfied is an after-invention; and in his letter to me he makes no complaint of that kind, but alludes to ſomething elſe, which he ſays I *I know*, but concerning which I can only form conjectures.

When I conſider every thing relating to this buſineſs, I cannot eaſily ſatisfy myſelf with any hypotheſis to account for Dr. H——s's behaviour to me. He is a man altogether unknown to the world. He has not diſtinguiſhed himſelf by any philoſophical diſcovery that I have yet heard of, and the airs he may give himſelf in his claſs, or in converſation, are nothing

nothing to the world at large. He may, in fact, be as great a man as Lord Bacon, Sir Iſaac Newton, or Mr. Boyle; and if his performances ſhould correſpond to the idea that his printed *Syllabus* is calculated to give us of him, he muſt be a greater man than any of them, and indeed greater than all the three put together. But then this cannot be known to the world, till his experiments, proving the diſcoveries that he has announced, be actually made, and an account of them he publiſhed, which will require at leaſt ſome months (though before that time his *ſubſcribers* may have an opportunity of knowing whether he be, in fact, the great man that he gives himſelf out to be or not; and ſome of them, it can hardly be doubted, will have *zeal* or *indiſcretion* enough to whiſper the fame of their maſter, whatever injunction his modeſty may lay them under) and during the time that I had the honour of his acquaintance, he had not ſo much as announced his importance to the world; for his famous *Syllabus* was not then publiſhed; ſo that even now, and much more ſo far back as the ſpring of the preſent year, he muſt be conſidered as *an obſcure perſon*, to whom, conſequently, the countenance of a perſon more known to the world might be ſuppoſed to be of uſe.

Now, with reſpect to myſelf, whether it has come to me by inheritance, or by acquiſition, juſt or unjuſt, whether it is owing to good fortune, or deſert, it is *fact*, that I have been ſome years in poſſeſſion of the moſt reſpectable acquaintance that this country can furniſh; and as it is almoſt univerſally true of Engliſh philoſophers, that they are much more celebrated abroad than at home, this has, of courſe, been the caſe with myſelf as well as others, and, by ſome accident or other, perhaps in a greater proportion with reſpect to me than moſt others; in conſequence of which, being naturally warm, and I will add conſtant in my attachments, it could not but be much in my power to befriend any man in the ſituation of Dr. H——s; who, one would imagine, would, therefore, rather wiſh to be brought forward by my friendſhip, than raſhly make me his enemy.

I therefore frankly acknowledge that I cannot clearly account for the fact, as a *phenomenon in human nature*; unleſs perhaps by adding to the conjectures abovementioned, that he may have been practiſed upon by ſome of my enemies (for all men have enemies) or that, being poſſeſſed of an uncommon degree of conceit, and having but little knowledge of the

the world and of mankind, to counteract the abſurd effects of that prepoſterous paſſion, he has taken it into his head, that he ſhould gain more by ſetting himſelf up as my *rival* in philoſophical reputation, than he ſhould do by availing himſelf of my friendſhip.

It appeared to me at the time, that he ſuſpected me not to be quite ſincere in what I had told him concerning my endeavours to ſerve him among my acquaintance, becauſe they did not immediately wait upon him, or attend his lecture; which was a very unreaſonable expectation. For a perſon who knows any thing of the world muſt have been apprized that, recommendations of this kind can only operate ſlowly, and that ſufficient time muſt be allowed in all caſes of this nature. For, at the ſame time that he told me that he was come to a reſolution to anſwer no more queſtions but ſuch as he was paid for, he ſaid, " a greater miſchief could not be done " to a man, than to flatter him with falſe ex- " pectations of patronage and encouragement." After this I reſolved not to do him any more miſchief of this kind. But neither have I done him any miſchief of a different kind; for I have never taken the leaſt ſtep to his prejudice. But, with reſpect to all theſe conjectures, I can only ſay with Logicians, *valeant quantum valere poſſunt.*

I ſhall conclude this ſection with acknowledging that this affair has contributed not a little to lower me in my own eſtimation, as I really imagined that my character was ſuch, as could not but have been more reſpected by ſuch a man as Dr. H——s, and that independent of my recommendation of him, he would even have thought my philoſophical communications (of which all my acquaintance know me to be very liberal) a ſufficient recompence for the little ſervices that he could do me.

SECTION

SECTION III.

An account of what I ſaw, or heard, of a philoſophical nature with Dr. H——s.

I ſhall now proceed to recite the ſubſtance of all that I ſaw or heard, that bore any relation to philoſophy, in the very ſhort time that I ſpent with Dr. H——s; that the public may form ſome judgement of the probability of my having taken from him any thing that I have ſince publiſhed as my own. But really our converſation very ſeldom turned upon philoſophy; moſt of the time that I was with him being taken up with complaints of the vaſt expence he had been at, and the little proſpect that he had of getting his capital back again: tho' I muſt do him the juſtice to ſay, that he always ſpoke with the greateſt contempt of *money*, calling it, to uſe his own words, mere *dirt* and *traſh*, compared with *philoſophy*. There only remains ſome little doubt, whether, in this, he had a view to his own money, or to mine.

Of his firſt lecture (which, of courſe, conſiſted of introductory matter, proper for be-

ginners) I remember nothing but his producing a variety of diagrams, in order to explain the nature of chemical attraction and repulſion, which he ſeemed to do with ingenuity enough.

In his ſecond lecture, he did very little beſides attempting to exhibit my experiments on alkaline air: but his apparatus being very ill contrived, he did not ſucceed to his wiſh. He was particularly embarraſſed in conſequence of uſing very long glaſs tubes, filled with quickſilver: but he told us that it was neceſſary to have them of that length, that when the mercury had ſubſided to its natural level, there might be a vacuum in the top of the tube, for the alkaline vapour to expand itſelf in. But in this, not only is his *reaſoning* very abſurd, but the *practice* is liable to lead the experimenter into a miſtake, with reſpect to the real quantity of the air introduced into thoſe long tubes. For my own part, I have ſeldom made uſe, for the ſame purpoſe, of tubes any longer than about nine inches, which are certainly both more commodious and more uſeful; and though the quickſilver compleatly fills theſe ſhort tubes, it is neceſſarily diſplaced, and its room occupied by the aſcending air or vapour.

But

But though he ſucceeded ſo ill in this experiment on *air*, I conſidered that the ſubject was new, and that it is only long practice that gives dexterity, and inſures ſucceſs in things of this nature. I cannot, however, forbear expreſſing my ſurpriſe on this occaſion, that he ſhould adopt my own method of exhibiting the alkaline principle, if it only tended to make "that myſterious and ſur-"prizing, which, in the method that was "known before to all practical chemiſts, "was perfectly intelligible." In an addreſs calculated for *ſtudents*, he certainly ſhould have adopted a method the leaſt myſterious poſſible.

The firſt philoſophical converſation that I had with Dr. H——s was of his own introducing, in the preſence of Dr. Brockleſby, on his favourite topic of the *conſtitution of fixed air*, on which we each of us gave our different opinions; he maintaining that it conſiſts of common air and phlogiſton, and I diſſenting from that opinion. He maintained, however, that I had once been inclined to that hypotheſis, or ſomething like it, and appealed to my book. The book was then, and is now, before the public, who may ſoon be ſatisfied that it contains no marks of my having ever given the leaſt countenance to an opinion

opinion ſo evidently void of all probability. For philogiſticated air wants almoſt every diſtinguiſhing property of fixed air.

It is not imbibed by water, it does not turn the juice of turnſole red, it does not precipitate lime in lime-water (though, during the proceſs, there is a precipitation of fixed air from the common air, which I diſcovered, and gave an account of in my firſt volume) and laſtly, which makes as manifeſt a diſtinction between theſe two kinds of air as any, they differ very greatly in ſpecific gravity: for fixed air is conſiderably heavier, and phlogiſticated air a little lighter than common air.

The former was the diſcovery of Mr. Cavendiſh, and the latter was an obſervation of my own, mentioned in my firſt volume, but more exactly aſcertained in the ſecond. Dr. H——s, however, not having attended to this as he ought to have done, ſays, in his *Syllabus*, page 3, that, "phlogiſticated air does not great-" ly exceed pure air in ſpecific gravity." On the contrary, he will ſee in my ſecond volume, if he thinks it worth his while to complete his ſett of ſo *capital* a work, that, the purer air is, the heavier it is, and the more phlogiſticated, the lighter.

Before

Before Dr. H——s lays claim to the diſcoveries of others, I think he ſhould ſhew that his *miſtakes* are his own. For his notion that fixed air conſiſts of common air and phlogiſton is advanced by Dr. Rutherford, in his diſſertation on the ſubject, and I am told was the opinion of Dr. Cullen, from whom Dr. H——s actually had it. In thoſe gentlemen the idea was very pardonable, the ſubject not having been ſufficiently examined; but it has been ſo fully inveſtigated of late, that ſo groſs a miſtake concerning it is now abſolutely unpardonable; eſpecially in a perſon who pretends to be a teacher of philoſophy, and who is a ſupercilious cenſurer of others.

Our next converſation, which was likewiſe begun in the preſence of Dr. Brockleſby, was on the ſubject of *acids in the form of air.* I told him that I had purſued what I had before diſcovered on that ſubject much farther; having, particularly, made many experiments on the vitriolic acid air, which the readers of my ſecond volume will ſee were begun at Mr. Trudaine's in France, and compleated preſently after my return to England, before I had ſo much as heard of Dr. H——s; and that I only wanted proper ſubſtances from which to expel the other acids in the ſame ſimple form, and a proper fluid to confine the nitrous. For the

the *vegetable acid air*, he mentioned ſeveral things which he thought would anſwer, and among others, a *concentrated vinegar*, of his own preparing, which I took, as the cheapeſt of them; and by the help of it I immediately made the experiments deſcribed in the ſecond ſection of my ſecond volume, acknowledging, as will there be ſeen, from whom I had the preparation.

After this, I was a little ſurprized, when, in the laſt converſation that I had with him, he told me, as a new thing, that he had diſcovered the *vitriolic acid air*. I replied, "Do you not "remember that I told you that I had done "the ſame, the very firſt time that I was in "your company, and that I had materials for "a pretty large ſection on that ſubject, in-"tended for my ſecond volume?" To this he made not one word of reply.

In our firſt converſation on the ſubject of acid air, I aſked him whether he could find me any fluid ſubſtance that would not be affected by the nitrous acid, which my readers will know to have been a great *deſideratum* with me. After ſome pauſe, he told me he could, and mentioned *bees wax*. But upon trying it with the ſtrongeſt nitrous acid that he himſelf could procure me (and by which he ſaid

it

it would be the leaſt affected) it was all diſſolved by it, when it was a little heated, and therefore did not anſwer in the leaſt. I told him of the failure of this experiment; upon which he ſaid he believed that he *did* know what would anſwer, but he did not tell me. I imagined that he intended to proſecute the experiment himſelf, and therefore I urged him no farther on that head.

While we were talking on this ſubject, he ſhewed me his proceſs for making *ſpirit of nitre*, which was then going on, to prove that there is much *air* in that acid. But I had not time to conſider what I ſaw, and I can give no good account of it. My own experiments give me a very different view of the ſubject; and when I attended a courſe of chemical lectures, delivered at Warrington, by the ingenious Mr. Turner of Liverpoole, I was one who aſſiſted in making a quantity of ſpirit of nitre, in a manner not ſo expeditious, indeed, as that which I ſuppoſe is now generally uſed, but in which I am pretty confident there was no opportunity for any common air to get into the compoſition of it. I wiſh, however, to examine this proceſs more particularly, and I think myſelf happy in having, for this, and other chemical purpoſes, made more than one acquaint-

acquaintance, by means of whom I ſhall ſoon be able to gratify myſelf in this reſpect.

At one time that I called upon Dr. H——s, he had a proceſs going on by which he told me that he procured the *ſedative acid*, and I think he likewiſe ſaid, *in the form of air*. If he can ſhew any ſuch acid air, it is entirely his own. I have no ſort of pretenſion to it. On the contrary, I am at preſent inclined to believe that there is no ſuch thing.

As to the experiments which I have made on the *fluor acid*, I queſtion whether I had ſo much as begun them at the time that my acquaintance with Dr. H———s terminated. Theſe I was enabled to make by means of Mr. Woulfe, without whoſe generous aſſiſtance I could have done nothing on the ſubject, as my narrative will ſhew.

At the time of my introduction to Dr. H——s, I had the greateſt part of the materials for my ſecond volume, and I told him I ſhould ſoon make another publication on the ſubject of air; but that I wanted to complete two courſes, viz. on the extraction of air from various ſimple and compound ſubſtances, by a burning mirrour in quickſilver, and alſo by a mixture of ſpirit of nitre; and I had ſeveral preparations

preparations of him for thoſe purpoſes, as my narrative will witneſs. Having got an ounce of *mercurius calcinatus per ſe*, of Mr. Cadet, while I was at Paris, for the purpoſe of my experiments on dephlogiſticated air, which were begun long before that time, I would have had ſome of Dr. H——s; but found that (tho' he aſſured me I had every thing of him at prime coſt) he could not afford it ſo cheap as Mr. Cadet. I therefore deſired him to make me a quantity of *red lead*, from which ſubſtance I had got air about five times as good as common air. When I firſt mentioned this kind of air to him, he ſaid, "How do you know that it is ſo pure?" I told him it appeared to be ſo both by the teſt of nitrous air, and alſo by a mouſe actually living in it five times longer than in an equal quantity of common air: to which he made no reply.

The firſt time that I ſaw him after I had got the red lead, which he had made for me, he ſaid, in the following identical words, "You "get no air from red lead." I told him I did, and even air five times as good as common air, ſuch as I had mentioned to him before; but, ſaid he "you get no air from the "red lead that I made for you." I told him I did, and air of the ſame kind, though in a very ſmall quantity. After this it is impoſ-

ſible

ſible that he ſhould have any pretenſions to the diſcovery of *dephlogiſticated* air, which is the only diſcovery for which the evidence of Dr. Brockleſby can be pretended, and even that pretended evidence has intirely failed him.

I firſt diſcovered that I could make dephlogiſticated air, and conſequently *common air*, from ſpirit of nitre and earth, when I was at Calne, on the 30th of March 1775; which is a diſcovery that directly overturns Dr. H——s's doctrine, as laid down in his *Syllabus*, which does not admit of the convertibility of either *earth*, or *acid*, into air. Upon my return to London, after I had ſent my letter upon that ſubject to the Royal Society, I told him that I now knew what common air was, for I could actually make it myſelf; and at the ſame time I mentioned the compoſition. To this he made not one word of reply. Now the air which I ſhewed to Dr. Brockleſby, and which, he aſſerts to have been the ſame with ſome that he had before ſeen in Dr. H——s's courſe, was this very kind of air; having been made with different kinds of earth with ſpirit of nitre. Now that Dr. H——s ſhould actually have made a ſpecies of air, the compoſition of which, according to the doctrine of his ſyl-

labus,

labus, juſt now publiſhed, is abſolutely impoſſible, I think my readers muſt deem to be a little extraordinary. In ſhort, if this diſcovery concerning the conſtitution of atmoſpherical air, be not my own, nothing that I ever did can be ſo; and if it be not ſufficiently proved by theſe conſiderations, nothing of this kind is capable of being proved.

With reſpect to this miſtake, however, as well as that concerning the conſtitution of fixed air, Dr. H——s has nothing to boaſt; for the opinion that he maintains on this ſubject is the very ſame that has always been maintained by almoſt every body except myſelf. But ſo clear are the proofs that I have produced of it, from actual experiments, that I will venture to ſay, that if Dr. H——s himſelf does not embrace it very ſoon, giving up his favourite fundamental doctrine of the *elementary nature of air*, he will be as ſingular in his opinion, as I have hitherto been in mine. Complete as his knowledge is of all the *ſeven elements* of nature, comprehending the *omne ſcibile* of natural knowledge, his *ipſe dixit*, delivered in his oracular ſyllabus, is not of ſo much authority, except perhaps with himſelf, as that of fact and experiment.

So much is Dr. H——s's doctrine on the ſubject of air the reverſe of mine, which makes *freedom from phlogiſton*, exactly to correſpond to *purity of air*, that, in a converſation with me, he maintained that air might have *too little*, as well as *too much* phlogiſton. He did not think proper to explain himſelf on the ſubject; and I can only aſſure him that I know no ſuch air. Let him produce it if he can, *et erit mihi magnus Apollo.*

In the ſame converſation in which I told Dr. H——s that I had diſcovered the real conſtitution of atmoſpherical air, I told him that I thought I had alſo diſcovered the compoſition of *fixed air*. Upon this he ſmiled, with a kind of triumph, ſaying, "You are convinced then, "at laſt, that fixed air is a compound." I told him I was, becauſe I thought I had diſcovered in what it conſiſted, viz. ſome modification of ſpirit of nitre, and phlogiſton, and perhaps ſome other principle. Upon this ſubject I am ſtill in ſuſpence, waiting for more experiments. But allowing that I had changed my opinion, which I have never been averſe to acknowledge, I have not yet adopted *his opinion*, viz. that fixed air conſiſts of common air and phlogiſton; ſo that I am no convert of *his*, but to *myſelf*, the opinion being, as far as I know, peculiar to myſelf; and therefore Dr. H——s

H——s can have no foundation for giving out, as I am informed he has done, that I have changed my opinion on the ſubject of fixed air, in conſequence of the converſation I had with him.

So far was Dr. H——s from being communicative to me of his knowledge, that he was not always in the humour, notwithſtanding his liberal promiſes, to let me have the materials with which he could have furniſhed me for my own experiments, except on ſuch terms as he ſaw I could not comply with. I once wanted a ſmall quantity of ſuch *phoſphorus* as Mr. Canton made; and as I ſaw that he had juſt made a quantity, of the excellence of which he boaſted very much (as, indeed, he did of almoſt all his preparations) I begged that he would let me have a little of it. He ſaid I ſhould, if I would promiſe to give no part of it to any body elſe. I told him that I had no intention of communicating it to any body, but that I did not like to lay myſelf under the obligation of ſuch a promiſe; and therefore I had none. Going to work myſelf, and following Mr. Canton's directions, I found no difficulty in making it ſufficiently well for my purpoſe.

SECTION IV.

Obſervations on Dr. H——s's Syllabus, as far as it relates to the doctrine of air.

In order to throw as much light as I poſſibly can on the ſubject which I have undertake to diſcuſs, viz. whether it be probable that I have borrowed any of my experiments of Dr. H——s, it may be uſeful to conſider whether his doctrine concerning *air*, contained in his Syllabus, lately publiſhed, be ſuch as may be ſuppoſed either to have *ſuggeſted*, or to have *reſulted from* thoſe experiments. If our *concluſions* be totally repugnant, it will hardly be thought probable that our *premiſes* were the ſame. Now that our concluſions are totally repugnant, will be evident to any perſon who ſhall inſpect his *Syllabus* and my *ſecond volume*; and it is ſomething remarkable that our opinions are, in no reſpect, ſo much the reverſe of each other, as in what relates to that very ſpecies of air, the diſcovery of which, the evidence of Dr. Brockleſby (if it could have determined any thing at all) would have given to Dr. H——s.

It was exceedingly fortunate for me, that Dr. H——s happened to publiſh this Syllabus of his, at this very ſeaſonable time; as, without it, my defence could not have been ſo complete as I am now able to make it; ſo that, without having ever thought of the matter, I find myſelf poſſeſſed of the earneſt wiſh of Job, *My adverſary has written a Book.* For now, out of his own mouth I can convict him; and ſo long as there remains a ſingle copy of that precious *Syllabus*, I muſt ſtand acquitted, and he condemned.

In this ſection I propoſe not only to point out the eſſential difference between Dr. H——s's opinons and mine, but, that my reader may derive ſome little advantage from the diſpute, I ſhall, as I have done in the preceding ſections, at the ſame time, ſhew how exceedingly frivolous are his objections to my doctrine, and how very crude, futile, and contrary to fact are his own; not forbearing to *laugh where we muſt*; ſince there is, in truth, very little room for *candour*.

In this curious ſyllabus, Dr. H——s repeatedly calls *acid air*, *alkaline air*, and *nitrous air*, p. 21, 27, *conceits*; alluding, no doubt, to myſelf, who firſt adopted theſe terms. Now

this muſt have ariſen from his ignorance of the *nature and uſe of words*, as if any perſon was not at liberty, (like Capt. Cook, or any other navigator) to call a thing which had no name before, by whatever name he pleaſed; or as if the *nature of the thing* was affected by the choice of a *term*. If inſtead of *air*, I had uſed the word *emanation*, *vapour*, *principle*, or Dr. H——s's more favorite term *element*, would there have been any real difference in the ſubſtance, ſo differently called? or, by calling them *air*, are the *facts* that I have diſcovered relating to them the leſs true.

Beſides, Dr. H——s himſelf uſes the term *inflammable air*, without the leaſt ſcruple, though, according to his theory, there is not a particle of air in that fluid. For he ſays, after me, p. 43, " that it conſiſts entirely of acid and phlogiſton." This was my own concluſion from the experiments mentioned in my firſt volume; but I have now rejected that opinion, becauſe I have ſince that time procured inflammable air from metals by *heat only*, without employing any *acid* whatever. Dr. H——s, however, is very welcome to keep my old opinion, if he prefers it to my new one. But which ſoever of the opinions he adopts, he is certainly obliged to me for it.

Not-

Notwithſtanding Dr. H——s thinks proper to call *nitrous air*, *acid air*, and *alkaline air*, mere *conceits*, and to conſider almoſt all my *originality* as a mere *knack to make plain things myſterious and obſcure*, I cannot help thinking that if the conceits had been *his own*, and if he himſelf had had as good a *knack* at theſe things as I have, he would have thought the conceits to be very pretty ones, and would have been not a little proud of his knack of ſtriking them out. And it is poſſible, that if he had produced any ſuch conceits of his own, he would not have looked with ſuch envious eyes on thoſe of others. On this account I really wiſh that he may have better fortune in his inquiries; for then, while he is exulting in his own diſcoveries, and *making mountains of mole-hills*, other quiet people may hope to enjoy their own property unmoleſted by him; unleſs he ſhould reſemble the lion in the fable, who, though he had no hand in catching the ſtag, challenged all the four quarters of it for himſelf.

I have obſerved that Dr. H——s has done me the honour to adopt ſeveral things from my firſt volume, but I ſee nothing common to us both of what is contained in my *ſecond* volume, except the mention of *vitriolic*, and

acetous air, which terms he heard me make uſe of, and which he calls *conceits*, and an intimation that he can explain the phenomenon of *detonation* without ſuppoſing a deſtruction of the acid. This was an eaſy and neceſſary reſult from ſome of my new experiments, eſpecially thoſe that relate to dephlogiſticated air, in the diſcovery of which he certainly had nothing to do.

His whole philoſophical theory reſts upon the foundation of there being diſtinct *primary elements* of matter, of which he makes ſeven, viz. *earth*, *water*, *alkali*, *acid*, *air*, *phlogiſton*, and *light*. All theſe, he aſſerts, p. 9, to be *impenetrable*, *immutable*, and *inconvertible*. But nothing can be more uncertain, or hazardous, than ſuch a poſition as this. We are far from being ſufficiently advanced in the knowledge of nature to pronounce concerning its primary conſtituent parts.

Dr. H——s more eſpecially aſſerts, p. 17, that *the pretended converſion of water into earth is an erroneous notion.* But while he pretends to have conſidered the experiments of Boyle, Borrichius, Wallerius, Leidenfroſt, Margraaff, Eller, and Lavoiſier (which is calculated to convey an idea of his extenſive reading) he has overlooked the more deciſive ex-

experiments of his countryman, the ingenious Mr. Godfrey, who converted the whole maſs of a conſiderable quantity of diſtilled water into a perfectly dry earth. For my own part, I ſee no reaſon to doubt of the fact; and what is much more, Mr. Woulfe, who is unqueſtionably one of the ableſt and moſt judicious chemiſts of the age, ſays that he has ſeen enough, in his own experiments, to make him perfectly ſatisfied with reſpect to it.

Dr. H———s calls *earth* an inconvertible element, but I will undertake to convert the whole of a quantity of earth into what he ſhall be obliged to call *air*; and, provided it be pure earth, by which I mean free from phlogiſton, it ſhall be reſpirable air.

He ſays, p. 44, that "the nitrous acid "prevents the formation of inflammable air, "in all circumſtances yet diſcovered;" whereas, if he had read my firſt volume with care, he would have found that, by a very eaſy proceſs, I can always make inflammable air from the nitrous acid, viz. by putting iron, or liver of ſulphur into nitrous air.

To mention a few other articles in this curious ſyllabus that do not relate to air, but ſome other of his elements: he ſays, p. 46, 50,

50, 51, that "fire conſiſts of light and phlo-
"giſton, and is not a certain motion of mat-
"ter; that *blaze*" (I ſuppoſe he means *flame*)
"is a mixture of fire and a phlogiſtic matter,
"which has not formed fire, p. 54; that light
"is not" (what Newton ſuppoſes) "a mat-
"ter ſent forth by the ſun, or ſtars, or pla-
"nets, p. 65; that darkneſs is not the ab-
"ſence of light, or any privation of light, ib.;
"that illumination, commonly called light,
"and darkneſs, are with reſpect to light,
"what ſound and ſtillneſs are with reſpect to
"air, p. 66; that our ſenſe of colour is our
"perception of the modifications of the vi-
"brating motions of light, and that the ſeven
"priſmatic colours are, with reſpect to light,
"what the ſeven tones are with reſpect to air,
"p. 72; that ſpecific gravity and denſity are
"not commutable terms; that there is not
"neceſſarily more matter in a cubic inch of
"glaſs, than in a cubic inch of rozin, for
"that gravity depends as much upon the ſpe-
"cies of the gravitating matters as upon the
"quantity of them, p. 48; that phlogiſton
"does not gravitate, and that it has a power
"whereby it counteracts the gravitation of
"other matters, p. 47."

Theſe

Theſe and ſuchlike long-exploded, and crude notions (ſo many of which I believe were never thrown together into the ſame compaſs ſince the age of Ariſtole or Carteſius) are delivered in a manner and phraſe ſo quaint, and a tone ſo ſolemn and authoritative, as gives me an idea that I cannot expreſs otherwiſe than by the term *Philoſophical Empiricism.*

SECTION

SECTION V.

MISCELLANEOUS OBSERVATIONS.

I ſhall begin this ſection with ſome general obſervations on the *nature of the accuſation* brought againſt me, and of the *evidence* by which it is ſupported.

My acquaintance with Dr. H——s commenced on the 6th of February 1775; and he ſays (for I happen not to have any note of that memorable æra myſelf) that it had been diſcontinued nine months, on the 3d of December following. It muſt, therefore, have terminated in the beginning of March. But I believe he is miſtaken about two months, and that it was in the beginning of May; ſo that I give him two months more than he claims. Three months, then (a great part of which I ſpent in the country) my acquaintance with Dr. H——s laſted.

The ſecond edition of my *Treatiſe on air* had been publiſhed ſome time before I had ſo much as heard the name of this gentleman, to whom it has been ſaid, and with very great confidence, that I owe *all my diſcoveries*; ſo that he can

can have no claim to any thing mentioned in that volume. At the ſame time, alſo, it is well known to my friends, and I mentioned it to Dr. H——s himſelf, the firſt time I ſaw him, that I had materials for a *ſecond publication* on the ſubject. I muſt, therefore, at that time, have had the materials for the *bulk* of the ſecond volume, I ſuppoſe about three fourths of it. The remaining fourth part, therefore, is all that can lie open to his claims; and even with reſpect to this, he will find that I am able to produce evidence, that every courſe of experiments, of any conſequence, was begun, and pretty far advanced, before I knew him; ſo that I had little to do beſides merely *completing* them, excepting what relates to the *vegetable acid air*, which is a thing of very little value, and the experiments on the *fluor acid*, which Dr. Brockleſby, the only evidence that has yet appeared againſt me, does not pretend to have ſeen with Dr. H——s.

In fact, therefore, there remains nothing of any value for him to lay his hands upon, except the completion of the diſcovery concerning *dephlogiſticated air*, which I had begun before I knew him; and though his friend has aſſerted, in general, that he ſaw *all* the experiments I ſhewed him (and theſe were among them) with Dr. H——s, the circumſtances of that

that fact have been ſtated to be ſuch, that I am ſatisfied my reader muſt be ſomething more than prejudiced, to imagine that it was even poſſible he ſhould have ſeen them.

When I firſt mentioned the facts to Dr. H——s, he even poſitively denied that any air could be got from the ſubſtance from which I actually procured that ſpecific kind of air; and the *neceſſary concluſions* from theſe experiments are not only not found in his printed *Syllabus*, but are the very reverſe of the fundamental doctrines of that ſyllabus.

Now I will venture to ſay that whenever any other article is examined, his claim to it will appear to be equally unreaſonable and abſurd. The book, however, will ſoon be before the public, and he may then caſt his rapacious eye over every paragraph of it; and let him diſtinguiſh his property there, if he can.

I am very confident, that if the dates annexed to any of the articles were concealed, and he was required to name his own, he would juſt as ſoon take what was done before I knew him, as what was done after that time. In fact, he has an equal right to *all*, or *none*.

It

It ſeems, however, very extraordinary to me, that he ſhould, at the ſame time, deſpiſe all that I have done, calling my diſcoveries mere *conceits*, and ſay that I am poſſeſſed of nothing but a *knack of rendering what was intelligible before, myſterious and obſcure*, and yet covet thoſe things for himſelf. The ſecond volume, I can aſſure him, contains nothing but *more conceits*, of the ſame kind with thoſe in the firſt, and nothing is exhibited in it but the exerciſe of *the ſame knack*, whether of darkening or enlightening things, that was diſplayed in the former volume.

According to Dr. H——s's account of the uſe that I have made of the diſcoveries of chemiſts, neither himſelf, nor any other perſon, has been really injured by me; for I have only diſgraced myſelf. What reaſon, then, can he have to complain? Let him only publiſh his experiments, which are ſo very intelligible; and if it appear, by compariſon, that mine are only calculated to throw darkneſs upon his light, their credit cannot laſt long; and every thing that I have done, contained in both my volumes, muſt vaniſh before his publication, like Satan, the prince of darkneſs, at the touch of Ithuriel's ſpear. If all that I have done be what he repreſents it, a mere *impoſition upon the public*, why cannot he

he be content that I ſhould have all the infamy of it to myſelf. Is it that he is willing, out of a principle of compaſſion, to ſhare the burden with me?

As he ſays that *I have treated others as I have treated him*, I think I may ſafely conclude, that I have only treated him, as I have treated others; and therefore that I have ſtolen no more from him, than I have done from others. Now, as my works are open to the public, let him ſhew what it is that I have taken from *others*, without acknowlegement. But as I am confident that all the world will acquit me of any thing like plagiariſm with reſpect to them, they will as readily acquit me of the ſame charge with reſpect to him.

During my acquaintance with Dr. H——s, he was perpetually talking of his *diſcoveries in general*, but without diſtinctly ſpecifying them; ſo that I do not retain a ſingle idea of any that he has ever made, and I have never heard the leaſt mention of any of them except from himſelf*. Indeed the great burden of his diſcourſe with me was, that people came perpetually teizing him with queſtions, took up

* I muſt except a ſingle circumſtance, mentioned in a late volume of the *Philoſophical Tranſactions*.

his time, got hints of diſcoveries from him, and then publiſhed them without making any acknowledgement. But I remember that he never mentioned the name of any of thoſe perſons. I now publicly call upon him to *name them*, that we may know one another, and compare notes; for I fancy we ſhall all find ourſelves in the ſame ſituation, that there has been *much cry and little wool*; that theſe *many perſons*, all publiſhers of experiments, have written from their own funds, and that we ſhould have had a very ſcanty ſupply, if we had only had Dr. H——s's hints, and voluntary communications, to depend upon.

Chemiſtry, however, being a wide field, and myſelf having had acceſs only to a one fruitful corner of it, I, in the great ſimplicity of my heart, entertained no doubt, but that while I was exploring one place, he was doing the ſame, and with the ſame ſucceſs in another; and there was certainly room enough for us all. But I now begin to ſuſpect that (whether through his too great eagerneſs to catch at every thing, and ſecure the whole field to himſelf, or through ſome other cauſe) not having had the good fortune, in fact, to lay hold of any thing himſelf, he has been ſeized with a longing deſire to ſnatch a few of

the flowers that I and others had been very quietly gathering; thinking that, out of ſo great a number, he might, without fear of detection, ſecure a few: and could he have contented himſelf with enjoying his pretenſions with more privacy and diſcretion, he might have ſucceeded better.

Now could we *all*, on whom he has been making his depredations, only know one of another, though we ſhould only be half a dozen of us (and yet, from his own account, which pretty much reſembled that of Falſtaff, I ſhould think that we cannot be leſs than a *ſcore*) we might perhaps, by conſulting together, hit upon ſome method of ſatisfying this unfortunate experimenter. We might each of us agree to make him a voluntarily contribution out of our common ſtock. For my own part, I love my reſt and peace ſo much, that rather than have ſuch another affair as this, I would freely ſurrender to him one or two leaves of my *Regiſter*, and a few good *hints* to work upon into the bargain.

Dr. H——s ſeems to be much offended at the *rapidity*, as he calls it, of my philoſophical publications. Now every man has a peculiar *manner*, and a peculiar *fate*. No two men are, in

in all reſpects, alike. He is not what I am, nor am I what he is. It may be my fate to be a kind of *comet*, or flaming *meteor* in ſcience, in the regions of which (like enough to a meteor) I made my appearance very lately, and very unexpectedly; and therefore, like a meteor, it may be my deſtiny to move very ſwiftly, burn away with great heat and violence, and become as ſuddenly extinct. Let Dr. H——s, therefore, if he be wiſe, keep out of my way; let him wait till my fated period arrive (which, in the nature of things, cannot be far diſtant) and he may then, after ſeeing my fall, like a ſlow ſober-moving *planet*, attended by his faithful ſatellite Dr. Brockleſby, perform his own revolution unmoleſted, when I ſhall be involved in darkneſs.

As a circumſtance that will have ſome weight with our judges, who are to decide whether it be more probable that the diſcoveries in queſtion be mine, or Dr. H——s's, I think he ſhould be required to produce before them any diſcoveries concerning air, that are *unqueſtionably his own*, as a ſpecimen of his abilities in this way; or, at leaſt, diſcoveries of ſome kind or other. Thus, when the waſps claimed the combs and the honey of the bees, they were required by their equitable judge, in Æſop, to produce ſuch combs themſelves.

I ſhall now conclude this appeal to the public with a letter to my *accuſer*, and another to his *witneſs*; after which I ſhall ſubmit the deciſion of my cauſe to a jury of our peers, the *public*; acknowledging, whether Lord Mansfield will agree with me in this, or not, that they are competent judges both of the *law*, and of the *fact*.

To Dr. H——s.

Sir,

It is ſomething odd that the ſubject of the only paragraph in my letter which you thought *required an anſwer*, is the only one which it is impoſſible for me to make a reply to in yours. For, indeed, as you ſay, *our notions of honour differ ſo very widely*, that it would be going too far back, for a correſpondence by letter, to come at any *common principles* on which we might argue. Beſides, the public may have notions of honour different from us both, and they will judge between us. To which of our maxims they will moſt incline, time will diſcover.

You ſay that, "if any other gentleman had "propoſed to you the queſtion that I did, an "anſwer

" anſwer would have been neceſſary." Now, as I cannot pretend to be any other perſon *now*, than I was *then*, I imagine you will ſtill think an anſwer to *me*, *unneceſſary*; but as with reſpect to the *public*, or to *yourſelf*, you may, poſſibly, think it *expedient*, and your time may be too much taken up in the proſecution of your immenſely valuable diſcoveries, even to read *the whole* of this pamphlet, I ſhall, in a few diſtinct paragraphs, recite all that it particularly concerns you to reply to.

1. You muſt diſtinctly recite thoſe diſcoveries of yours, which you charge me as having publiſhed as my own; proving that you had publiſhed them before me, and that *I knew* of your having made them at the time of my publication. You ſay that " If you ſhould anſwer my queſtion, you would commence it " with compariſons of the dates of my rapid " publications, with the dates of your courſes " of chemiſtry." Now this is more than neceſſary, unleſs you can prove that I knew any thing of you, or of your courſes, before the 6th of February laſt, and can find in the *two firſt lectures of the courſe*, which you began on that day, the ſeeds of my diſcoveries ſubſequent to that date.

But I find, by your friend Dr. Brocklesby, that your very *firſt courſe* began in June 1774; whereas my firſt volume on air was publiſhed ſome months before that date; ſo that, tho' your voice could have been heard from your laboratory in Greek-ſtreet, Soho, London, to my fire-ſide at Calne, in Wiltſhire, I could not have profited by your inſtructions. My unfortunate *conceits* were then all abroad, and, to my everlaſting ſhame, were at that time well known to philoſophers in many different parts of Europe; and long before that time, the Council of the Royal Society, wanting the wiſdom of your advice, had been ſo infatuated, as to have conferred upon me their annual prize-medal for about one half of thoſe that are contained in that firſt volume. Miſled by their ill-founded applauſe, I have gone blindly on in the ſame walk, till my conceits are now more than four times as many as they were at the time of my firſt publication.

2. After you have made good your *firſt* charge of plagiariſm, with reſpect to *yourſelf*, pleaſe to prove your ſecond alſo, viz. my plagiariſm with reſpect to *others*.

3. That

3. That this altercation of ours may be of ſome uſe to the public, and to make it worth their while to give us a hearing, I wiſh you would diſcuſs the ſeveral topics on which your philoſophical notions and mine differ. It is to be wiſhed, more eſpecially, that you would prove your favourite doctrine, that fixed air conſiſts of common air and phlogiſton; that acid air, alkaline air, and nitrous air, &c. &c. &c. are mere *conceits*; and that your fundamental principle of the abſolute inconvertibility of what you call *elements* into each other, is well founded, eſpecially that *earth* is not convertible into *air*, as I aſſert, and you deny.

4. It would be particularly edifying to the public, if you would favour them with an elucidation of your extraordinary *Syllabus*, a few things in which I took the liberty to point out, as *wanting ſome illuſtration*; as your notions concerning *fire*, *light*, *phlogiſton*, &c. But perhaps you may, in your great prudence and diſcretion, think it quite ſufficient, if, for the preſent, you can give ſatisfaction to your *pupils* with reſpect to them: and I own, upon reflection, it would be unreaſonable to require of any perſon of your deſcription, that he ſhould publiſh to the world all the *ſecrets* of his *Art*.

5. As to the particulars which only paſſed *in converſation* between our two ſelves, and which have not yet been communicated to the public, not even in your amazingly-comprehenſive ſyllabus, as concerning the *ſedative acid, air that has too little phlogiſton*, &c. &c. &c. it no way concerns the public; but perhaps you may chuſe, while your hand is in, to clear up thoſe matters as well as the reſt.

6. Above all things I muſt inſiſt upon it, that you ſpecify the *names* of the *many perſons* who have behaved towards you with the ſame baſeneſs and ingratitude that I have done; that the public may judge of the credibility of your charge againſt me, by comparing it with your charge againſt others, probably much more reſpectable perſons than myſelf.

7. As to your perſonal *behaviour* to me, and your *reaſons* for it, you may give juſt what account you pleaſe. As my character is pretty well known, thoſe who are acquainted with me will judge whether your account be probable or not; and though I do not pretend but that my memory may fail me with reſpect to ſome *circumſtances* of things, I think a man will hazard too much who ſhall

ſhall charge me with any *wilful* miſrepreſentation of a fact. I would not for the ſake of all the reputation that a man can get by philoſophy, or by writing, have the feelings of that man who ſhall charge me with having told *a deliberate falſehood*; for if he have any knowledge of me, he muſt, at the ſame time, be conſcious of telling one himſelf; aſſerting what he does not believe.

I am, Sir,

Your humble Servant,

Shelburne-houſe,
8 Dec. 1775. J. PRIESTLEY.

P. S. As I have now the honour of introducing you to the Public, as Dr. Brockleſby introduced me to you, I hope you will not immediately adopt the ſtyle of your *letter* to me, with which our intercourſe terminated, but rather that which you firſt uſed to me, as expreſſive of that deference and reſpect which you thought due to a new acquaintance.

If in any part of this pamphlet, or letter, I have *inadvertently offended* you, I ſhall hope to be favoured with a *remonſtrance* on the ſubject. The *title*, at leaſt, cannot diſpleaſe you. In this I have not been partial to myſelf; for whenever

whenever the publication is mentioned, it will be called *my Philoſophical Empiriciſm*, and not *yours*. Your friends, therefore, may be expected to circulate it as well as mine.

To Dr. Brocklesby.

Dear Sir,

I am ſorry to obſerve that, in your laſt letter, you drop the uſual ſtile of ſtiendſhip, in your addreſs to me; but this ſhall not make me diſcontinue it with reſpect to you. For though, by means of your indiſcretion, I have long lain under a great load of odium, and you have occaſioned me a great deal of trouble in conſequence of it, I believe it was very far from being your intention to injure me: and whatever I may think of you as a *philoſopher*, or as a *writer*, I ſhall always reſpect you as a *gentleman*. Beſides, your known attachment to the cauſe of Liberty, would alone, if you had nothing elſe to recommend you to me, diſarm, in a great meaſure, my reſentment.

I have no objection even to your entertaining whatever regard you pleaſe for your friend Dr. H———s, who, how deep ſoever he may be in *philoſophy*, and how happy a talent ſoever he may poſſeſs of communicating

municating his own clear ideas to others, (of which yourſelf, who have ſo long been his pupil, have exhibited the moſt ſatisfactory proof) certainly has not, in return, received *from you* all the accompliſhments of a *gentleman*; being manifeſtly deficient in the firſt rudiments of that character, viz. *good manners*, as his *behaviour*, and his *letter* to me, will teſtify.

I would obſerve, however, that ſeveral parts of your letter might very juſtly give me offence, eſpecially your intimating that by propoſing my queſtions I meant not to *clear up the ſubject* (though nothing could be better calculated to anſwer the purpoſe, as the iſſue has proved) but either to *puzzle* you, or lead to *farther perplexities*. For they could not have puzzled or perplexed any man who was a competent evidence in the caſe.

All the particulars of your letter that are *true* (for ſeveral of the articles are notoriouſly falſe) are either things that have been long known to all philoſophers, or are contained in my firſt volume; and yet, after reciting them, you bid me "aſk myſelf whether any "thing that I ſhewed you at Shelburne-houſe "could appear novel to you; ſince my giving

"new

"new names to what you had ſeen before, "could not conſtitute any new diſcovery." Now I ſhewed you ſeveral very remarkable experiments, of which nothing is ſo much as hinted at in your letter; and therefore, on your own teſtimony, they muſt have been quite new to you, whether you were aware of it or not.

You complain that you have been drawn by me into a diſpute againſt your will; but, Dear Sir, is it not rather you who have drawn *me* into this diſpute? And I do aſſure you it is much againſt my will. A very ſerious accuſation has been brought againſt me, reſpecting, not, as you repreſent it, *the excluſive right to a philoſophical diſcovery*, but affecting my *character as an honeſt man*, and you are the only perſon who have ſtood forth in ſupport of this accuſation. Can you then ſeriouſly blame me for calling you to prove what you acknowledge you have aſſerted, and for propoſing ſuch queſtions as were evidently neceſſary to aſcertain the validity of your teſtimony? A moment's reflection will convince you that, in juſtice to myſelf, I could not have done otherwiſe.

You

You muſt now, Sir, give me leave, in return for your anecdote concerning Sir Iſaac Newton (which you have intirely miſapplied in my caſe) to tell you a ſtory which you cannot miſapply, and I hope it will not be loſt upon you on a future occaſion.

A Chineſe Mandarine had procured an European reflecting teleſcope, and a friend of his, wiſhing to have another exactly like it, put it into the hands of a Chineſe workman, who was famous (as many of the Chineſe are known to be) for the imitation of any thing he ſaw. Accordingly, having got the inſtrument into his hands, he ſurveyed it with great attention, took it to pieces, and carefully meaſured the dimenſions of every part. He then made a tube of the very ſame ſize, and mounted and poliſhed it, ſo as not to be diſtinguiſhed from the other; and with reſpect to the inſide of it, he put pieces of poliſhed metal, and pieces of tranſparent glaſs in their proper places, and preciſely at their proper diſtances from each other; but without attending to any more exact curvatures of their ſuperficies than his eye, which was a very good one, could diſtinguiſh: and then concluded that he had completely conſtructed the teleſcope. And certainly a Chineſe Dr. Brockleſby

leſby would have ſaid that *they appeared to be nearly the ſame*; and yet the European inſtrument would magnify remote objects with great diſtinctneſs, whereas, through the Chineſe teleſcope, nothing could be ſeen at all.

Now this I take to have been the difference between Dr. H——s's experiments and mine, and I hope that the next time that you ſhall ſee a man ſtanding by a tub of water, or a baſon of quickſilver, with jars and phials, &c. before him, filled partly with air and partly with water, with a lighted candle, and a variety of little implements at hand, and transferring his different kinds of airs, with ſome degree of dexterity, from one veſſel to another, a *red* colour appearing here, and a *white* one there, you will not be ſo ready to affirm that the operator was *inſtituting* the very ſame experiments that you ſaw at Shelburne-houſe.

As a ſtory frequently begets its own likeneſs, and examples of this kind may be uſeful to you in the way of *apology*, as well as of *admonition*, I ſhall, while I am in the vein for it, tell you another.

Your expreſſing no ſort of *ſurprize* at ſeeing my new experiments, reminds me of the in-

indifference with which Tobiah, a very ſenſible native of Otaheite (ſo that it is no diſparagement to you to be compared to him) ſaw a *horſe* for the firſt time at Batavia, when it was imagined that he would have been ſtruck with the greateſt admiration, eſpecially as he was remarkable for his curioſity, and his attention to every thing that appeared new to him. But when he was aſked by Mr. Banks, who told me the ſtory, if he did not admire that noble animal? he ſaid, "No, for there "was nothing extraordinary in it, except its "ſize, as ſuch animals were common enough "in his own country." Upon inquiry it appeared that he took the horſe to be nothing more than a large dog.

Unfortunately, this Otaheitian Dr Brockleſby did not live to return to his own country. But ſuppoſing him to have returned, and his countrymen gathered about him, aſking him whether he had ſeen any thing *new* in his travels; he would have ſaid, "No, theſe peo-"ple (who are ſaid to be *philoſophers high in* "*modern rank*) with great trouble and ex-"pence, run over the world, on purpoſe to "make what they call *diſcoveries*, and I ſup-"poſe there are people on whom they can im-"poſe. But as far as I ſee, and I have been

"long

" long enough with them to judge, they only
" amuſe themſelves, and the world, with *giv-*
" *ing new names to things* that we all know as
" well as they do. They pretended to ſhew
" me a moſt extraordinary animal, and thought
" to have ſurprized me exceedingly with the
" ſight of it; but though they called it by a
" name that I had never heard before, and
" that I cannot now recollect, you may depend
" upon it, it was nothing more than a *dog*,
" only a little larger than our dogs generally
" are. It had only four legs like ours, one
" head, one tail, and a couple of ears, and it
" ſeemed to run at the ſame ſpeed. As for the
" *ſpecies* of the animal, let them pretend what
" they will, be aſſured by me it was the very
" ſame."

I hope, Dear Sir, you will not think the worſe of me for endeavouring to give a turn of pleaſantry to an affair that, ſome time ago, wore a pretty ſerious aſpect. Believe me, I retain no animoſity againſt you. I have even no objection to ſeeing or converſing with you as uſual. Only I fancy it will be equally agreeable to us both to ſay nothing, for the future, about philoſophy, or Dr. H——s, but rather

to talk about America, and our common friend Dr. Franklin.

I am, Dear Sir,

Your very humble Servant,

Shelburne-houſe,
Dec. 9, 1775. J. PRIESTLEY.

P. S. Had you liſtened to the gentle and timely admonition of Dr. Hunter, at the time of your making the declaration abovementioned, viz. "I ſuppoſe that what Dr. H——s "ſhewed might be *ſomething like* theſe experi- "ments, but they might notwithſtanding, in "reality, be very different from them," it would have been happy for me, and perhaps alſo for yourſelf, and even for your friend Dr. H——s, in whoſe reputation you ſo warmly intereſt yourſelf.

THE CONCLUSION.

I have now made the beſt defence that I can to the *general* and *indiſtinct charge* that has been brought againſt me, and am waiting (with how much anxiety may well be imagined) for the *particulars* of my accuſation, of the nature of which I am juſt as ignorant as my reader himſelf. I take it for granted, however, that it relates to ſome of the articles contained in Dr. H———s's ſyllabus, which was intended, no doubt, to compriſe the reſult of all his diſcoveries, thoſe that I have been pilfering, as well as the reſt; and indeed it takes in the whole compaſs of philoſophical knowledge. But then, among ſuch an immenſe number of diſcoveries, great and ſmall, how can I determine which of them it is on which he will found his charge.

I believe I muſt, in this caſe, have recourſe to the method formerly uſed in taking the *ſortes Virgilianæ*; and, as it is poſſible, though not through *a deficiency*, yet through a *redun-*

dancy of his articles of accuſation (which is no leſs embarraſſing) Dr. H——s himſelf may be as much at a loſs as I am, I would recommend the ſame method to him; and if he ſhould not happen to know what it is (as the proceſs is not a ſtrictly chemical one) I will tell him, that he has nothing to do but to open the book at random, and the firſt paragraph that he ſhall caſually caſt his eye upon, is the article wanted.

I am not lawyer enough to know whether it would avail me at all in this caſe, to turn *informer* againſt my proſecutor, or I could prove that not a ſingle article mentioned in Dr. Brockleſby's elaborate letter (which I doubt not contains a full and accurate account of all the *recondite* doctrines, and profound diſcoveries, delivered in the Greek-ſtreet lecture) whether *true* or *falſe* (for the account conſiſts of a due mixture of both) belongs to Dr. H——s.

If this will not avail me, and my defence, after all, be deemed unſatisfactory, I ſhall be anxious to know to what *puniſhment* I ſhall be ſentenced. For if my crime ſhould be adjudged to be any thing more than *petty larceny*, I am apprehenſive that, as we have no co-

lonies for the convenience of tranſporting felons now, I ſhall inevitably be *deſtined to the cord.*

I do not know whether my *nativity* was ever regularly *caſt*; but if it was, I am confident it muſt have appeared, that I was born under the malignant influence of ſome or other of the *planets*, to which the old chemiſts paid a more particular devotion; and it is well known that they had much recourſe to the planets. For I cannot otherwiſe account for my being ſo exceedingly obnoxious to *lecturers in chemiſtry* as I have been. If I might adopt the doctrines of my Scotch antagoniſts, I ſhould ſay they ſeem to be poſſeſſed of an *inſtinctive antipathy* towards me, and to fall upon me as naturally as the wild aſſes, in Arabia, fall upon the horſe, or, if they like it better, as the wild horſes of Arabia fall upon the aſs.

For, a few years ago, I happened to be but a quarter of an hour in company with another celebrated lecturer in this branch of liberal ſcience, in this metropolis, and I narrowly eſcaped being brought into a ſcrape as bad as this that I am now in with Dr. H——s.

It

It was, indeed, much of the ſame nature, and, as far as I can foreſee, would have ended as this is likely to do. Nay it looked much more formidable at its outſet. For I was informed not only that I had publiſhed diſcoveries communicated to me in that unlucky quarter of an hour, without any acknowledgement, but a publication was threatened of *all my plagiariſms*, which, as I was then but young in this buſineſs (and not caſe-hardned, as I now am by all kinds of abuſe) would certainly have overwhelmed me. And the learned lecturer (though I believe he never acted in concert with Dr. H——s) expreſſed even greater contempt of my experiments than Dr. H——s has done, and in a ſtile equally correct and elegant.

When, however, the fact was inquired into, it only appeared, that I had not given to an excellent philoſopher, with whom I am now better acquainted, an experiment, which, as the chemiſt deſcribed it, was *not fact*, and which, as it ought to have been deſcribed, I had not claimed to myſelf, but had given to another perſon, who had actually made the experiment, and had publiſhed an account of it long before.

Having

Having related the particulars of my own conduct, and my own experiments, as far as the purpose of my defence requires,

——— *Sua narret Ulysses.*

OVID.

FINIS.

ERRATA.

Page 6. l. 4. (from the bottom) for *none*, read *none of them*.
P. 4. l. 11. for *ſubſtance*, read *ſubſtances*.
P. 58. l. 5. for *not*, read *not yet*.
P. 59. l. 4. for *Ariſtole*, read *Ariſtotle*.

A CATA-